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Using Real-Time Data to Test for Political Budget Cycles

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Abstract

We use real-time annual data on the fiscal balance, government current spending, current revenues and net capital outlays as published at a half yearly frequency in the OECD Economic Outlook for 25 OECD countries. For each fiscal year t we have a number of forecasts, a first release, and subsequent revisions. It turns out that revisions in the fiscal balance data are not affected by elections. However, we do find that governments spend more than reported before an election which provides support for moral-hazard type of political budget cycle (PBC) models: through hidden efforts the incumbent tries to enhance his perceived competence. We also find that governments had higher current receipts than reported before an election, which is in line with adverse-selection type of PBC models in which incumbents signal competence through expansionary fiscal policy before the elections.

JEL Classification: D72; E62; H6; H83; P16.

Key words: real-time data, political budget cycles, OECD

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1. Introduction

Since the seminal work of Orphanides (2001), Croushore and Stark (2001) and Orphanides and van Norden (2002), several papers have applied real-time data analysis to monetary policy. Real-time data analysis refers to research for which data revisions matter or for which the timing of the data releases is important in some way (Croushore, 2011). Despite the surge in real-time data analysis, the standard procedure in the macroeconomic and political science literature on modeling economic policies is still to use latest-available data. However, as pointed out by Croushore (2011), this approach is based on the heroic assumptions that data are immediately available (when in fact they are generally available only with a lag) and that data revisions either do not exist or are inconsequential small (when in fact they are often large and may significantly affect empirical results). Analyzing policy using today's data set is almost certain to be misleading as it gives the researcher no sense of the data that policymakers had available when they made decisions. This is illustrated by the work by Orphanides (2001) that shows for the Federal Reserve that policy recommendations based on real-time data differ considerably from those obtained with ex-post data. Orphanides also finds that estimated policy reaction functions based on ex-post data yield misleading descriptions of historical policy. Using Federal Reserve staff forecasts, simple forward-looking specifications describe policy better than comparable Taylor-type specifications based on ex-post data. This is further exemplified by the work of Gorter et al. (2008), who report similar results for the ECB's policies.¹

Even though problems related to data revisions and the timeliness of information clearly matter also for fiscal policy, research using real-time data for fiscal policy analysis only came up in recent years. In his survey of this literature, Cimadomo (2011) divides the literature on fiscal policy and real-time data into three main groups. The first group includes papers analyzing deviations of ex-post outcomes from estimates of fiscal variables. An example of a study in this category is the work of de Castro et al. (2011), who focus on 15 EU countries covering the period 1995-2008. Their results indicate that revisions of deficit data are frequent. Furthermore, preliminary deficit data releases are biased and non-efficient predictors of subsequent releases, with later vintages of data tending to show larger deficits on average. The authors also find that expected real GDP growth, political cycles, and the strength of fiscal rules contribute to explain revision patterns.

¹ Sauer and Sturm (2007), on the other hand, report that the use of real-time data in Taylor rule estimates for the ECB does not play such a significant role as in the case of the Federal Reserve.

The second group of studies includes papers on the determinants of forecast errors, defined as deviations of ex-post outcomes from governments' fiscal plans for the next year. According to Cimadomo (2011), deviations of fiscal outcomes from government plans are mainly influenced by macroeconomic forecast errors, due to model uncertainty or unexpected shocks. Still, some papers find evidence that also political factors play a role. For instance, in their analysis of one-year-ahead forecast errors for the general government deficit for a panel of 15 euro-area countries plus Japan and the US for the period 1995-2003, Brück and Stephan (2006) find that right-wing governments tend to make more pessimistic forecasts. In addition, they find that minority governments tend to make overly optimistic forecasts.

The third group of studies includes papers on the evaluation of the ex-ante vs. ex-post cyclical stance of fiscal policies, i.e., on the reaction of fiscal policies to business cycle fluctuations. These papers test whether fiscal policies have exerted a stabilizing influence on the business cycle or whether they have tended to exacerbate economic fluctuations (Cimadomo, 2011). An example is the study of Cimadomo (2007) who employs a dataset of 19 OECD countries constructed from past issues of the OECD Economic Outlook for the period 1994-2006. He finds that fiscal policies are counter-cyclical, especially during times of expansions.

Our paper deviates from previous work in two respects. First, most previous studies analyze differences between fiscal plans and final outcomes. A good example is the study of Beetsma et al. (2009) who decompose fiscal adjustments into planned and actual adjustments. Instead of focusing on forecast errors, we focus on revisions in net lending, current spending and current revenues. Second, we focus on the impact of elections on these revisions in net lending, current spending, current revenues and net capital outlays. Even though there is substantive evidence that fiscal policy is used by incumbents to increase their chances for re-election (see section 3 for a discussion of some recent studies), only very limited attention has been given to the role of elections in the literature on fiscal policy and real-time data. An exception is the study of Pina and Venes (2011) for EU countries for the period 1994-2006. These authors find that one-year-ahead forecast errors for the government deficit

are affected by upcoming elections that tend to induce over-optimism.² Similarly, De Castro et al. (2011) report that the proximity of an election leads a government to delay a revision towards a higher deficit.

Our analysis is based on real-time annual data on the fiscal balance, government current spending, current revenues and net capital outlays as published at a half yearly frequency in the OECD Economic Outlook for 25 OECD countries, covering the period 1997-2006.³ For this sample period we have data on forecasts of fiscal policy variables, nowcasts (i.e. the figures released at the end of the year concerned), first releases (i.e. the first figures released after the year has ended) as well as subsequent revisions of these figures. We find that revisions in the fiscal balance data are not affected by elections. However, when distinguishing between current spending and revenues, we do find that governments spend more than reported before an election. This provides support for moral-hazard type of political budget cycle (PBC) models: through hidden efforts the incumbent tries to enhance his perceived competence. We also find that governments had higher current receipts than reported before an election, which is in line with adverse-selection type of PBC models in which incumbents signal competence through expansionary fiscal policy before the elections.

The remainder of the paper is structured as follows. Section 2 describes the data used. Section 3 offers a brief review of recent literature on political budget cycles and presents our hypotheses. Section 4 contains our empirical results, while section 5 offers a sensitivity analysis. Section 6 contains our conclusions.

2. Data

We make use of the data set as published in the OECD Economic Outlook. We focus on annual data published at a half yearly frequency for 25 OECD countries. For each fiscal year we have data on the fiscal balance (government net lending) as well as data on general government current spending

² Beetsma et al. (2009) report results for three political variables in their empirical model for budget implementation errors, namely a variable capturing a change in the party composition of the government, the number of government changes, and an election dummy. Added one-by-one, these variables turn out to be significant. However, including these three variables jointly, the election dummy ceases to be significant and is, therefore, dropped in their main model.

³ The last vintage we use has been published in the autumn of 2011. In order to capture the revision process the final year to which we refer is 2006.

(disbursements), current revenues (receipts) and net capital outlays.⁴ For each variable, we have for every fiscal year t a number of forecasts (published before or during the year t), a first release (published in the June issue of the Economic Outlook in year $t+1$), and subsequent revisions (published in December of $t+1$ and June of $t+2$, etc.). The (nominal) fiscal variables are scaled by nominal GDP as forecasted in spring of $t-1$ for the year t . This allows us to interpret the ratios in the usual way, whereby at the same we take care of differences in country size and currencies used. Furthermore, the use of a fixed release eliminates the direct effect on the ratios due to revisions in GDP data. The release is chosen as such that it is always available when the remaining data used is published. Figure 1 illustrates the structure of our data set. Take, for example, information referring to the year 1997. The OECD provides forecasts (and nowcasts) for the fiscal data of 1997 in the June and December issues of the Economic Outlook of 1996 and 1997. The published figures are forecasts, provided by the fiscal authorities of the country concerned, as the fiscal year 1997 has either not started (in 1996) or not finished (in 1997) at the time the figures are released. These forecasts and nowcasts are indicated in Figure 1 by F2, F1, and F0, respectively. In the June 1998 Economic Outlook, the OECD publishes the first estimate of the budget balance in 1997 (indicated by R1 in Figure 1). In subsequent issues of the Economic Outlook, new figures for the 1997 budget balance are released, provided by the Statistical Office of the country concerned, indicated by R2, R3, etc. in Figure 1. We use data up to the 8th release of figures for t published in the autumn of $t+4$. The OECD Economic Outlook issue 60, published in December 1996, is the first one for which all relevant time series are available. The last vintages we use are published in the OECD Economic Outlook issue 90, published in December 2011. Consequently, the years for which we have all relevant vintages are 1997-2006.

[Insert Figure 1 about here]

Figure 2 illustrates for the case of the Netherlands how the released figures for the budget balance evolve over four years according to subsequent issues of the Economic Outlook. Take the balance for fiscal year 2002 (straight purple line in Figure 2). According to the forecast in the December 2001 Economic Outlook, the 2002 budget balance would be positive, but the nowcast in the December 2002 issue projected a small deficit. According to the first release as published in June 2003, the 2002 deficit was higher than forecasted (1.1% of GDP), and subsequent revisions led to further increases in the deficit (to 2% of GDP). As a prelude to the analysis presented in section 4, it is worthwhile pointing out

⁴ These three components exactly add up to government net lending. To be more precise, net lending equals receipts minus disbursements minus net capital outlays.

that elections took place in the Netherlands in the spring of 2002, indicated by the blue vertical bar in Figure 2. The reported figures on the Dutch deficit deteriorated only after the elections took place. Naturally, it might have been the case that early projections as well as first official releases on the budget balance were based on incomplete data. Yet, political budget cycle theory, to be discussed in section 3, suggests that such a phenomenon also may reflect opportunistic behavior of incumbents who try to manipulate data in order to raise their re-election probability.

[Insert Figure 2 about here]

Figure 3 shows that data revisions (in this case: the difference between the first and the sixth release of the budget balance) vary within and across countries. Revisions can be both upward and downward. In 15 out of 25 countries the median revision (shown within each box) is downward, meaning that most of the time the fiscal balance turns out to be worse than initially reported. Greece has the highest median value for downward revisions. On the other hand, Luxembourg has the highest median value for upward revisions. Revisions are sometimes quite substantial as shown by the dots in the figure. These dots are revisions that fall outside the whiskers, i.e., the ends that represent the lowest and highest revision values that are within 1.5 times the interquartile range. Norway had the biggest downward revision, while the UK and Iceland had the biggest upward revisions that do not fall within the whiskers. The outer bounds of the boxes represent the lower and upper quartiles (i.e. contain 50% of the observations).

[Insert Figure 3 about here]

As said, our database also contains information about government current spending, current revenues and net capital outlays, which allows us to decompose the net lending revisions. Figure 4 shows some results, again taking the revisions between the first and the sixth release. Current spending revisions, on average, lead to downward revisions of net lending (i.e. a higher deficit), except for Luxembourg, Japan and Slovakia. Current revenue revisions lead, on average, to upward revisions for net lending, except in Greece, Japan, Portugal and Slovakia. It is noteworthy that except for Denmark, Greece, Luxembourg and Portugal the revisions in current spending and current revenues go in opposite directions and thereby often more or less cancel each other out.

[Insert Figure 4 about here]

3. Political budget cycles: recent studies and hypotheses⁵

The notion that incumbents have an incentive to use economic policies in order to enhance their chances on re-election goes back to Nordhaus (1975). Ever since, many studies have focused on PBCs. The evidence is rather mixed. For instance, Shi and Svensson (2006) report significant pre-electoral increases in the government budget deficit for their panel of 85 developing and developed countries over the period 1975-95. Moreover, Persson and Tabellini (2002) find statistically significant tax decreases before elections in their sample of 60 democracies over the period 1960-98. However, according to Brender and Drazen (2005), the results of the studies of Shi and Svensson (2006) and Persson and Tabellini (2002) are driven by the experience of so-called 'new democracies', where fiscal manipulation may be effective because of lack of experience with electoral politics in these countries. They argue that once the 'new democracies' are removed from the sample, evidence in support of the PBC disappears.

However, also several more recent studies focusing on 'established democracies' find evidence for the existence of a PBC. For instance, Tujula and Wolswijk (2007) find support for a PBC in their sample of OECD countries for the period 1975-2002. Mink and De Haan (2006) provide similar evidence for European Union (EU) member states after the start of the monetary union. Similarly, Efthyvoulou (2011) reports for the 27 EU member states over the period 1997-2008 that incumbent governments across the EU tend to manipulate fiscal policy in order to maximize their chances of being re-elected. He finds that the relative importance of non-economic issues prior to elections and the uncertainty over the electoral outcome can to a large extent explain the variability in the size of PBCs across and within the EU countries.

Most recent empirical studies on PBCs pay little attention to the theoretical motivation of election effects in fiscal policy (a clear exception being Shi and Svensson, 2006). As pointed out by Shi and Svensson (2003), three generations of theoretical PBC models can be distinguished. The first-generation models emphasize the incumbent government's intention to secure re-election by maximizing its expected vote share at the next election (Nordhaus, 1975). It is assumed that the electorate is backward looking and evaluates the government on the basis of its past track record. As a result, these models imply that governments, regardless of ideological orientation, adopt expansionary fiscal policies in the late year(s) of their term in office in order to stimulate the economy. The second-

⁵ This section draws on Klomp and De Haan (2012).

generation models – Shi and Svensson (2003) call them adverse-selection-type models – emphasize the role of temporary information asymmetries regarding the politicians' competence level in explaining electoral cycles in fiscal policy. In these models, signaling is the driving force behind the PBC (see e.g. Rogoff and Sibert, 1988). It is assumed that each political candidate has a competence level (high or low) that is known only to the politician and not to the electorate. Voters want to elect the more competent politician and form rational expectations regarding the incumbent's type based on observable current fiscal policy outcomes. Before the election, high-type incumbents will attempt to signal their type (and thereby increase their chances of re-election) by engaging in expansionary fiscal policy, which is less 'costly' for them than it is for low-type incumbents. Third-generation PBC models are based on moral hazard. Examples of such studies are Persson and Tabellini (2000) and Shi and Svensson (2006). As in the adverse-selection models, each politician has some competence level that is unknown to the electorate. Additionally, it is assumed that politicians cannot observe their own competence level *ex ante* either. That is, politicians are uncertain about how well they will be able to handle future problems. Voters are rational and therefore want to elect the most competent politicians because that would imply higher efficiency levels of post-election public goods production. The constituents' inference is based on the observable macroeconomic performance of the incumbent government. The key assumption in moral hazard type of models is that the incumbent government can exert a hidden effort, that is, use a policy instrument unobservable to the public that is a substitute for competence. For example, if competence measures how well politicians can convert revenues into public goods, the hidden effort can be interpreted as the government's short-term excess borrowing. Elections take place after the incumbent government's hidden effort and competence have jointly determined the observable macroeconomic outcome. The incumbent government would like to increase its performance index by exerting more effort, hoping that voters will attribute the boost in public goods provision or the lowering of the tax burden to its competence. In equilibrium, there will be an excessive effort on the part of the incumbent politicians, and, as a result, there is a higher degree of expansionary fiscal policy prior to an election.

The use of real-time data makes it possible to test implications of the second- and third-generation PBC models. Both types of models have different implications for the time pattern of fiscal policy data revisions. In adverse-selection models, voters are rational and there is asymmetric information with respect to the competence level of the incumbent politician between the politician and the electorate. To reveal his type, a good politician will engage in expansionary fiscal policies before elections, as this is less costly to him than to his bad counterpart. This leads to a separating equilibrium in which the good politician reveals his type and will be elected. However, it is questionable whether the

equilibrium in this model is truly separating. If a politician can mimic that he is a good politician by reporting higher expenditures or lower receipts and subsequently adapts government statistics once elected, he reaches his objective without further (short-term) costs. So the incumbent signals high competence by producing statistics showing expansionary fiscal policy. Over time, however, it will be difficult to maintain these biased accounts and after the election the degree of expansionary fiscal policy as shown in fiscal data will start to reflect actual policy implying *decreases* in net lending over time via downward revisions in government disbursements or upward revisions in government receipts. In moral-hazard models, the incumbent pursues expansionary fiscal policy before the election, but hides these efforts by not reporting them in preliminary statistics. Only after the election will it become clear what the incumbent did so that the degree of expansionary fiscal policy shown in fiscal data *increases*. So adverse-selection and moral-hazard models have different implications for the pattern of fiscal policy data revisions. In the next section, we will analyze the impact of elections on fiscal policy data revisions in order to examine whether they provide evidence for either or both types of PBC models.

4. Estimation results

It is common in research on real-time data to estimate variants of the following model:

$$(1) \quad \Delta y_{i,j}(t) = y_j(t) - y_i(t) = \alpha + \beta y_i(t) + \mu(t)$$

where $\Delta y_{i,j}(t)$ denotes the revision of a fiscal policy variable for period t between release i and j . Under the Mincer-Zarnowitz (1969) test for forecast efficiency the null hypothesis to be tested is: $\alpha = \beta = 0$. If data are revised only if new information becomes available, i.e.

$$(2) \quad y_j(t) = y_i(t) + \varepsilon_{i,j}(t), \quad \text{cov}(y_j(t), \varepsilon_{i,j}(t)) = 0 \text{ for } j > i$$

and if the error term $\varepsilon_{i,j}(t)$ is orthogonal to earlier releases, revisions have a zero mean (i.e. $y_i(t)$ is an unbiased estimate of $y_j(t)$). In other words, under the null hypothesis, future values are unpredictable at the time of announcement.

In our panel data context, we expand equation (1) by including country-fixed effects (α_c), as well as a dummy for data published within a year before elections (E), and the revision of GDP growth between i and j ($\Delta g_{c,i,j}(t)$).⁶ So the model estimated is:

$$(3) \quad \Delta y_{c,i,j}(t) = \alpha_c + \beta y_{c,i}(t) + \gamma \Delta g_{c,i,j}(t) + \phi E_c(t) + \mu(t), \quad \text{where } j > i$$

We collected data on election dates using the Political Data Handbook (PDH) of the *European Journal of Political Research* (EJPR). For those countries that are not covered by the PDH, we relied on <http://electionresources.org/>. The election dummy variable in our regression model is equal to one when there is an election scheduled to take place within the next 12 months after the fiscal data was published and zero otherwise.⁷

[Insert Table 1 about here]

Table 1 shows the results for revisions in government net lending. In the first column the dependent variable is the difference between the last forecast before the start of fiscal year t (published in the Economic Outlook of December of t-1) and the sixth revision (published in the Economic Outlook of December of t+2). In the second column the dependent variable is the difference between the nowcast (published in the Economic Outlook of December of t) and the sixth revision. The remaining columns show the results using the difference between the sixth and higher revisions and the first release (published in the Economic Outlook of June t+1) as dependent variable. The results in Table 1 show that apart from the growth revisions in the first few columns none of the variables is significant. The forecasts of net lending turn out to have been too optimistic when growth forecasts turn out to have been too optimistic, but neither the initial value nor elections have explanatory power for future revisions. As Figure 4 suggests that revisions in current spending and current revenues show an offsetting pattern, we have also estimated the same equation using the three underlying components of net lending as dependent variable. Tables 2, 3 and 4 show the results.

[Insert Table 2 about here]

[Insert Table 3 about here]

⁶ Although the revision of GDP growth is not known at period i (or before period j), it is included to produce a more precise estimate of the standard errors. For obvious reasons, revisions in GDP growth are likely to explain revisions in our fiscal variables.

⁷ We experimented with different publication leads but this does not affect the qualitative results.

[Insert Table 4 about here]

Whereas for revisions in net capital outlays (Table 4) the election variables remain insignificant, the results in Tables 2 and 3 provide support for an election effect. Table 2 shows that with hindsight governments did spend more than reported before an election. This provides support for moral-hazard type models: through hidden efforts the incumbent tries to enhance his perceived competence. Only after the elections it becomes clear that fiscal policy was more expansionary than initially reported. Table 3 shows that governments also had higher current receipts than reported before an election. From that perspective, fiscal policy was less expansionary than reported before the election. This outcome is in line with adverse-selection type models in which incumbents signal competence through expansionary fiscal policy before the elections.

[Insert Table 5 about here]

Confirming Figure 4, Table 5 reports a high contemporaneous correlation between especially the residuals of the equations explaining disbursement and receipts revisions. To produce more efficient estimates, we use a seemingly unrelated regression approach as suggested by Zellner (1962). We also carried out a Breusch-Pagan test of independence. The null hypothesis of zero correlation between the residuals is very clearly rejected implying that we will indeed get more efficient estimates using a system estimator.

[Insert Table 6 about here]

Indeed, as the results in Table 6 show, whereas the coefficient estimates for most variables remain in the same order of magnitude, the estimated standard errors turn out to be overall smaller, i.e. we obtain more precise estimates. This, however, does not affect our qualitative results. We still find that when elections are approaching the first release government spending data for the year that has ended turn out to be on average 0.7 percentage points of GDP lower than reported two and a half years later. Government receipts are initially reported to be on average 0.8 percentage points of GDP lower. For net capital outlays we do not find an election effect.

5. Sensitivity analysis

So far, we have included all elections in our analysis. However, one could argue that the incentive for governments to manipulate government statistics is reduced when it is able to determine the election date. Exogenously determined elections, on the other hand, could create an environment in which it is more attractive for the incumbent to manipulate fiscal statistics. As a first sensitivity analysis, we therefore differentiate between exogenous and endogenous elections. Following Shi and Svensson (2006), an election date is classified as exogenous if either (i) the election is held on the fixed date (year) specified by the constitution; or (ii) the election occurs in the last year of a constitutionally fixed term for the legislature; or (iii) the election is announced at least a year in advance. On the basis of this definition, our data set contains 66 elections of which 45 are exogenous and 21 endogenous.⁸

[Insert Table 7 about here]

The results in Table 7 indicate that although the coefficient estimates for the exogenous elections are estimated with higher precision than those for endogenous elections, we cannot statistically distinguish between the two.

A second check that we have done is to examine whether there is a difference between elections after which the incumbent is replaced by a new government and elections in which the government is re-elected.⁹ Arguably, hidden efforts are more likely to become visible because a new government can blame its predecessor for the revisions. In contrast, a re-elected government may have few incentives to revise data as it may have used fiscal policy to get reelected. On the other hand, if the incumbent government can be quite sure to be reelected then there is no need to manipulate its financial accounts.

[Insert Table 8 about here]

⁸ We have used the data set of Potrafke (2012) to distinguish between exogenous and endogenous elections. He has collected data using the definition of Shi and Svensson (2006) for most OECD countries. Since our sample differs from the sample of Potrafke, we categorized exogenous and endogenous elections using the PDH of the EJPR. For the cases not covered by the PDH, we relied on <http://electionresources.org/> and crosschecked the results using the definition of Brender and Drazen (2005).

⁹ Under our definition, there is a new government if there is a new (coalition) government, which has a different ideological orientation than its predecessor. This is better approach than the commonly applied rule to define a new government based on whether or not there is a new prime minister. Even though the prime minister may remain the same, the coalition (and therefore the ideological orientation of the government) may have changed. Likewise, the coalition may remain in power after the elections, but there may be a new prime minister.

Out of the 66 elections that are relevant in our estimation sample 34 are classified to have induced a change in the government. In 32 cases no change in the government has occurred. We again do not find significant differences between these two types of elections (see Table 8).

6. Discussion and conclusions

There is a small but fast-growing literature in which real-time data are used to model behavior of policy-makers. Analyzing policy using ex post data set may be misleading as it gives the researcher no sense of the data that policymakers had available when they made decisions. Using real-time annual data on the fiscal balance, government current spending, current revenues and net capital outlays as published at a half yearly frequency in the OECD Economic Outlook for 25 OECD countries, we examine whether data revisions are affected by upcoming elections. It turns out that data revisions vary within and across countries. In 15 countries the median revision is downward, meaning that most of the time the fiscal balance turns out to be worse than initially reported. Current spending revisions, on average, lead to downward revisions of net lending (i.e. a higher deficit), while current revenue revisions lead to, on average, upward revisions for net lending (i.e. a lower deficit). In most countries the revisions in current spending and current revenues go in opposite directions and thereby often more or less cancel each other out.

We leave it to future research to explain the variability across countries and over time in more detail. Several possible explanations come to mind, like differences in political and budgetary institutions that could affect the costs of cheating, cultural differences (like risk aversion and long-term orientation), and political polarization. Some preliminary results (not reported here but available on request) suggest that differences in institutional quality (proxied by quality of the bureaucracy) and the political system in place (presidential vs. parliamentary systems) seem to matter.

Our results suggest that revisions in the fiscal balance data are not affected by elections. However, we do find that governments spend more than reported before an election which provides support for moral-hazard type of political budget cycle (PBC) models: through hidden efforts the incumbent tries to enhance his perceived competence. We also find that governments had higher current receipts than reported before an election, which is in line with adverse-selection type of PBC models in which incumbents signal competence through expansionary fiscal policy before the elections.

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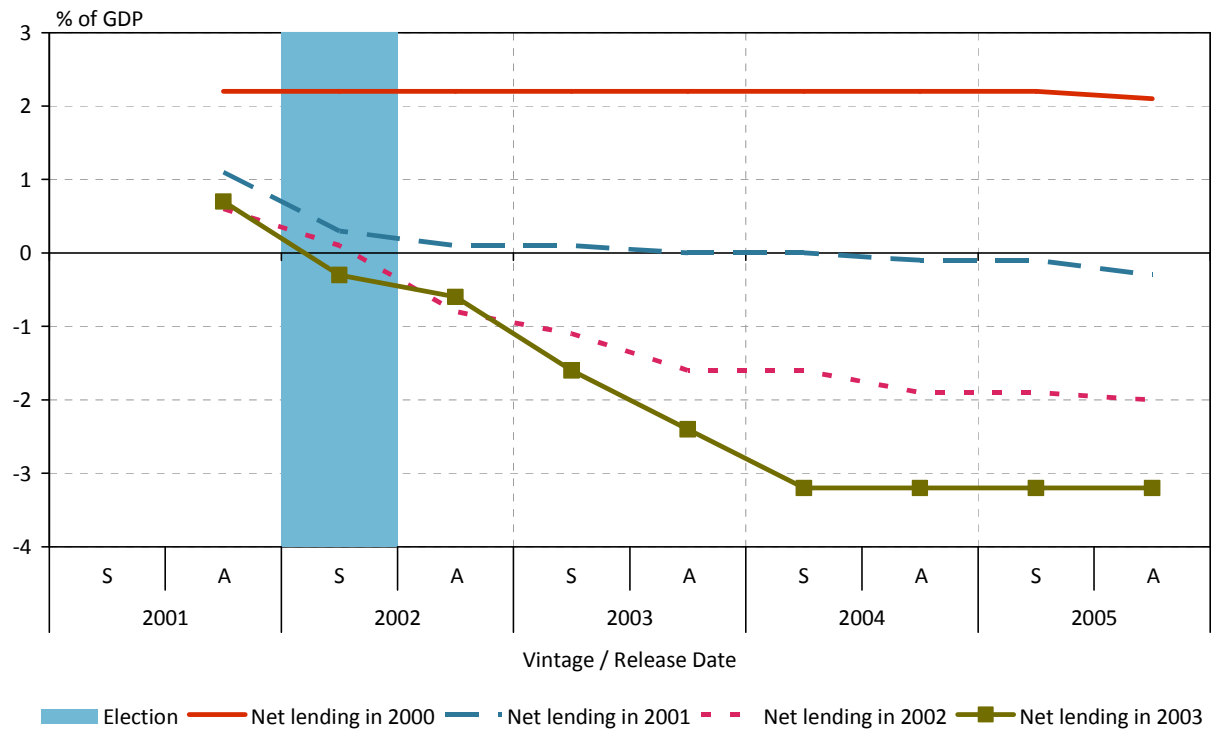
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Figure 1: Data structure

| | | Vintage / Release Date | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
|------------------|------|------------------------|-----|------|-----|------|-----|------|-----|------|-----|------|-----|-----|------|-----|------|-----|------|-----|------|-----|------|-----|------|-----|-----|----|-----|----|-----|----|----|----|----|----|----|----|----|--|--|--|--|--|--|--|
| | | 1996 | | 1997 | | 1998 | | 1999 | | 2000 | | 2001 | | ... | 2006 | | 2007 | | 2008 | | 2009 | | 2010 | | 2011 | | | | | | | | | | | | | | | | | | | | | |
| | | Jun | Dec | Jun | Dec | Jun | Dec | Jun | Dec | Jun | Dec | Jun | Dec | ... | Jun | Dec | Jun | Dec | Jun | Dec | Jun | Dec | Jun | Dec | Jun | Dec | | | | | | | | | | | | | | | | | | | | |
| Reference Period | ... | ... | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 1997 | F2 | F1 | F0 | R1 | R2 | R3 | R4 | R5 | R6 | R7 | R8 | ... | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 1998 | F4 | F3 | F2 | F1 | F0 | R1 | R2 | R3 | R4 | R5 | R6 | ... | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 1999 | | | F4 | F3 | F2 | F1 | F0 | R1 | R2 | R3 | R4 | ... | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 2000 | | | | F4 | F3 | F2 | F1 | F0 | R1 | R2 | ... | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 2001 | | | | | | | F4 | F3 | F2 | F1 | F0 | ... | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 2002 | | | | | | | | | F4 | F3 | F2 | ... | R7 | R8 | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 2003 | | | | | | | | | | | F4 | ... | R5 | R6 | R7 | R8 | | | | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 2004 | | | | | | | | | | | | | ... | R3 | R4 | R5 | R6 | R7 | R8 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | 2005 | | | | | | | | | | | | | | | ... | R1 | R2 | R3 | R4 | R5 | R6 | R7 | R8 | | | | | | | | | | | | | | | | | | | | | | |
| | 2006 | | | | | | | | | | | | | | | | | ... | F1 | F0 | R1 | R2 | R3 | R4 | R5 | R6 | R7 | R8 | | | | | | | | | | | | | | | | | | |
| | 2007 | | | | | | | | | | | | | | | | | | | ... | F3 | F2 | F1 | F0 | R1 | R2 | R3 | R4 | R5 | R6 | R7 | R8 | | | | | | | | | | | | | | |
| | 2008 | | | | | | | | | | | | | | | | | | | | | ... | | | F4 | F3 | F2 | F1 | F0 | R1 | R2 | R3 | R4 | R5 | R6 | | | | | | | | | | | |
| | 2009 | | | | | | | | | | | | | | | | | | | | | | | ... | | | | F4 | F3 | F2 | F1 | F0 | R1 | R2 | R3 | R4 | | | | | | | | | | |
| 2010 | | | | | | | | | | | | | | | | | | | | | | | | | ... | | | | | F4 | F3 | F2 | F1 | F0 | R1 | R2 | | | | | | | | | | |
| 2011 | | | | | | | | | | | | | | | | | | | | | | | | | | | ... | | | | | | F4 | F3 | F2 | F1 | F0 | R1 | R2 | | | | | | | |
| 2012 | | | | | | | | | | | | | | | | | | | | | | | | | | | | | ... | | | | | | | F4 | F3 | F2 | | | | | | | | |
| 2013 | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | | ... | | | | | | | | F4 | | | | | | | |

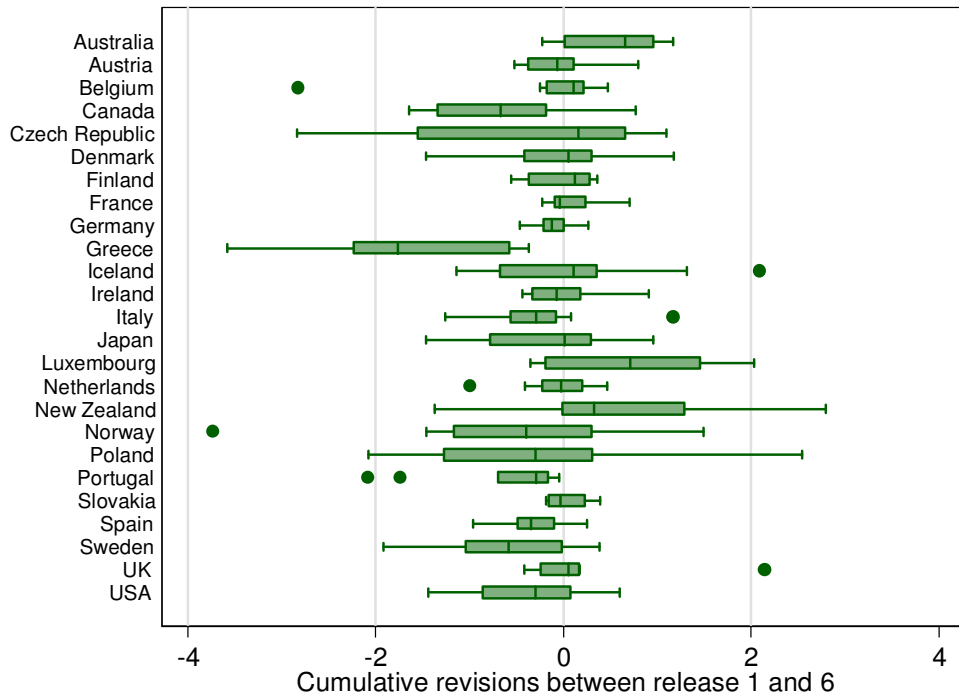
Notes: “R” stands for “Release” and is followed by the release number. “F” stands for “Forecast” and is followed by a number indicating how many publication periods are left before the reference period has been realized.

Figure 2: Case study the Netherlands – reported government net lending for the years 2000, 2001, 2002 and 2003



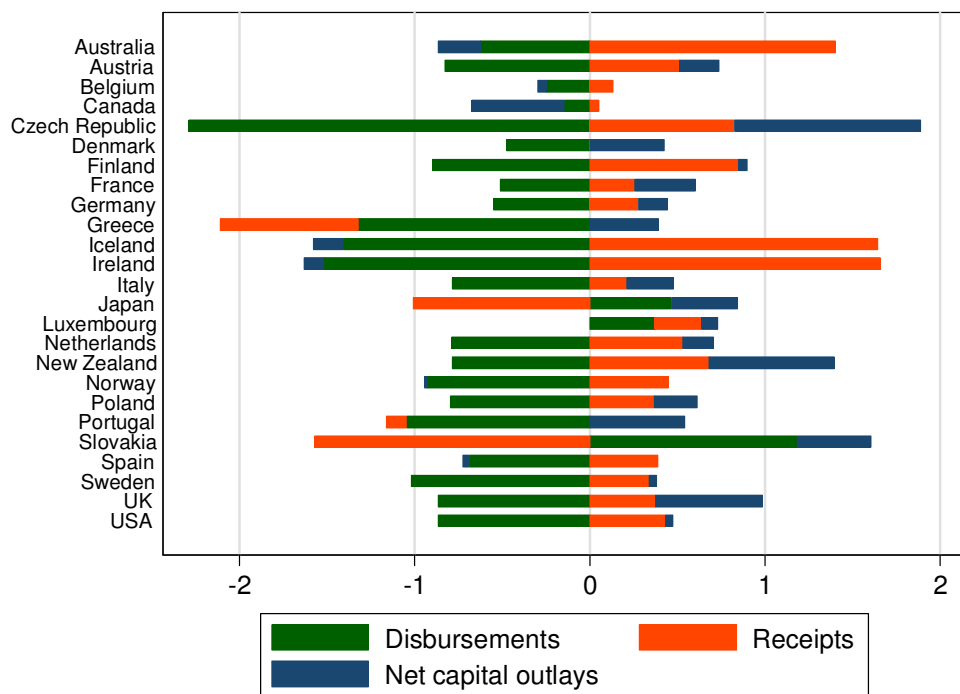
Note: The 2002 elections in the Netherlands took place on May 15.

Figure 3: Distribution of revisions in government net lending data across countries



Notes: The bottom and top of the boxes represent the lower and upper quartiles. The median is shown within each box. The ends of the whiskers represent the lowest and highest values which are still within 1.5 times the interquartile range. Data not included between the whiskers are shown as dots.

Figure 4: Decomposition of average revisions in government net lending across countries



Notes: The sum of the average revision in government disbursements, government receipts and net government capital outlays equals the average revision in government net lending.

Table 1: Explaining revisions in government net lending

| | Forecast F2 -> R6 | Nowcast F0 -> R6 | Official statistics R1->R6 R1->R7 R1->R8 | | |
|--|----------------------|----------------------|---|---------------------|---------------------|
| Initial value, $y_{c,i}(t)$ | -0.197* (-1.965) | -0.0265 (-0.707) | -0.0172 (-0.552) | -0.0150 (-0.447) | -0.0117 (-0.362) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | 0.454*** (3.510) | 0.178** (2.754) | 0.130* (1.792) | 0.130 (1.421) | 0.139 (1.534) |
| Elections within 12 months, $E_c(t)$ | -0.146 (-0.640) | -0.0170 (-0.0998) | 0.0667 (0.557) | 0.0971 (0.833) | 0.150 (1.256) |
| Observations | 238 | 238 | 238 | 238 | 238 |
| R-squared | 0.443 | 0.267 | 0.261 | 0.293 | 0.304 |

Notes: Robust t-statistics in parentheses. Standard errors are clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Explaining revisions in government disbursements

| | Forecast F2 -> R6 | Nowcast F0 -> R6 | Official statistics R1->R6 R1->R7 R1->R8 | | |
|--|----------------------|---------------------|---|-------------------------|---------------------|
| Initial value, $y_{c,i}(t)$ | -0.0676 (-0.664) | -0.0589 (-0.763) | -0.0587 (-0.944) | -0.0709 (-0.957) | -0.0729 (-1.034) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | -0.0914 (-0.842) | -0.268* (-1.837) | -0.0794 (-0.449) | -0.000539 (-0.00310) | 0.110 (0.586) |
| Elections within 12 months, $E_c(t)$ | 0.278 (1.565) | 0.150 (0.554) | 0.800*** (3.039) | 0.509** (2.665) | 0.503** (2.534) |
| Observations | 238 | 238 | 238 | 238 | 238 |
| R-squared | 0.389 | 0.257 | 0.259 | 0.290 | 0.341 |

Notes: Robust t-statistics in parentheses. Standard errors are clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Explaining revisions in government receipts

| | Forecast F2 -> R6 | Nowcast F0 -> R6 | Official statistics R1->R6 R1->R7 R1->R8 | | |
|--|----------------------|-----------------------|---|---------------------|---------------------|
| Initial value, $y_{c,i}(t)$ | -0.0226 (-0.277) | -0.0181 (-0.320) | -0.0251 (-0.496) | -0.0336 (-0.597) | -0.0358 (-0.623) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | 0.425** (2.687) | -0.00911 (-0.0726) | 0.0577 (0.279) | 0.183 (0.717) | 0.208 (0.798) |
| Elections within 12 months, $E_c(t)$ | 0.463** (2.246) | 0.0450 (0.177) | 0.665** (2.702) | 0.375* (1.757) | 0.478** (2.147) |
| Observations | 238 | 238 | 238 | 238 | 238 |
| R-squared | 0.484 | 0.266 | 0.143 | 0.146 | 0.170 |

Notes: Robust t-statistics in parentheses. Standard errors are clustered by country. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Explaining revisions in government net capital outlays

| | Forecast F2 -> R6 | Nowcast F0 -> R6 | Official statistics R1->R6 R1->R7 R1->R8 | | |
|--|-----------------------|-----------------------|---|-----------------------|-----------------------|
| Initial value, $y_{c,i}(t)$ | -0.681*** (-4.155) | -0.572*** (-4.315) | -0.474*** (-5.235) | -0.528*** (-6.036) | -0.560*** (-5.993) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | 0.0143 (0.626) | 0.0835** (2.124) | 0.0507 (0.581) | 0.0843 (0.959) | 0.00592 (0.0829) |
| Elections within 12 months, $E_c(t)$ | 0.250* (1.782) | 0.0727 (0.570) | -0.0289 (-0.290) | -0.0414 (-0.451) | 0.0211 (0.188) |
| Observations | 238 | 238 | 238 | 238 | 238 |
| R-squared | 0.551 | 0.489 | 0.483 | 0.548 | 0.571 |

Notes: Robust t-statistics in parentheses. Standard errors are clustered by country. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Correlation coefficients of regression residuals

| | Disbursements | Receipts | Net capital outlays |
|---------------------|---------------|----------|---------------------|
| Disbursements | 1 | | |
| Receipts | 0.836 | 1 | |
| Net capital outlays | -0.186 | 0.091 | 1 |
| Net lending | -0.066 | 0.247 | -0.230 |

Notes: Correlations of regression residuals are based upon 238 observations.

Table 6: Seemingly Unrelated Regression estimate of components underlying government net lending

| | R1 -> R6 | | |
|--|------------------------|------------------------|-----------------------|
| | Disbursements | Receipts | Net capital outlays |
| Initial value, $y_{c,i}(t)$ | -0.0528*** (-3.423) | -0.0592*** (-3.952) | -0.297*** (-9.247) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | 0.102 (0.609) | -0.0789 (-0.492) | 0.0492 (0.744) |
| Elections within 12 months, $E_c(t)$ | 0.727*** (2.714) | 0.801*** (3.119) | -0.0735 (-0.691) |
| Observations | 238 | 238 | 238 |
| R-squared | 0.135 | 0.259 | 0.438 |

Notes: t-statistics in parentheses. Equations are estimated using seemingly unrelated regression estimation (Zellner 1962) while iterating over the estimated disturbance covariance matrix and parameter estimates until the parameter estimates converge, thereby converging to maximum likelihood results. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Regression results when distinguishing between different types of elections

| | R1 -> R6 | | | R1 -> R6 |
|---|------------------------|------------------------|-----------------------|---------------------|
| | Disbursements | Receipts | Net capital outlays | Net lending |
| Initial value, $y_{c,i}(t)$ | -0.0524*** (-3.370) | -0.0591*** (-3.925) | -0.297*** (-9.240) | -0.0169 (-0.541) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | 0.101 (0.601) | -0.0794 (-0.494) | 0.0499 (0.755) | 0.129* (1.788) |
| Endogenous Elections within 12 months, $E_c(t)$ | 0.768* (1.690) | 0.835* (1.911) | -0.133 (-0.735) | 0.130 (0.740) |
| Exogenous Elections within 12 months, $E_c(t)$ | 0.707** (2.234) | 0.786*** (2.591) | -0.0470 (-0.377) | 0.0385 (0.245) |
| Observations | 238 | 238 | 238 | 238 |
| R-squared | 0.135 | 0.259 | 0.438 | 0.261 |

Notes: t-statistics in parentheses. The first three equations are estimated using seemingly unrelated regression estimation (Zellner 1962) while iterating over the estimated disturbance covariance matrix and parameter estimates until the parameter estimates converge, thereby converging to maximum likelihood results. The last equation is estimated with standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Regression results when distinguishing between elections followed by changes in the government

| | Subsequent government changes | | | R1 -> R6 |
|--|-------------------------------|------------------------|-----------------------|---------------------|
| | Disburs. | Receipts | N.cap.out. | N.lend. |
| Initial value, $y_{c,i}(t)$ | -0.0531*** (-3.439) | -0.0595*** (-3.975) | -0.297*** (-9.247) | -0.0169 (-0.543) |
| Growth revision, $\Delta g_{c,i,j}(t)$ | 0.104 (0.616) | -0.0707 (-0.440) | 0.0426 (0.646) | 0.131* (1.781) |
| Elections within 12 months, $E_c(t)$ and change | 0.704** (1.997) | 0.643* (1.901) | 0.0609 (0.436) | 0.0354 (0.209) |
| Elections within 12 months, $E_c(t)$ and no change | 0.752** (2.070) | 0.970*** (2.783) | -0.216 (-1.502) | 0.0998 (0.705) |
| Observations | 238 | 238 | 238 | 238 |
| R-squared | 0.135 | 0.261 | 0.443 | 0.261 |

Notes: t-statistics in parentheses. The first three equations are estimated using seemingly unrelated regression estimation (Zellner 1962) while iterating over the estimated disturbance covariance matrix and parameter estimates until the parameter estimates converge, thereby converging to maximum likelihood results. The last equation of each block is estimated with standard errors clustered by country. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.



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